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GULF STREAM DRIFT MISSION PRESENTATION

(This transcript was prepared from a tape recording.)

P R O C E E D I N G S

VOICE: -- especially in a mission like this is weeding through the data and determining what to present. And today we're going to present some of the preliminary results, and we're very fortunate to have on hand Dr. Jacques Picard, who not only originated the submersible but was very instrumental in getting the mission going.

We also have Chet May, who was the NASA participant in the mission.

We will hear from them shortly. But first I would like to give you a little background of the mission. And I only have three or four Vu-graphs, so you don't have to worry about sweating through a long presentation by me.

The first Vu-graph shows the trace of the mission. It started some twenty miles off of Palm Beach and ended up some three hundred miles south of Nova Scotia and about five hundred miles from land, thirty days after initiation.

You notice one break in the curve, in the middle, and that's where they met some unforeseen circumstances and they had to surface due to being caught in an eddy outside their mission, outside of the Gulf Stream.

The depth along the traverse varied from a nominal six hundred feet, and they had several excursions to fourteen hundred feet.

The mission was thirty days.

The purpose was twofold: one was to make scientific observations, and the other was simply to insure that the submersible was adequate.

So we had two types of people involved: we had the engineering type, and we had the scientific type, that made up the crew.

The crew size was six, and it had an international flavor. There were two Swiss, there was a Britisher, and there were three Americans. And the Americans were quite unique because one was an oceanographer, one was captain of the ship, and the other was a NASA representative.

The NASA involvement was completely exploratory. There was a feeling that there might be some -- that there could be some reasonable interface between this kind of mission and a long duration space mission.

The Vu-graph on the far side is a pictorial representation of tne craft. Some 48.8 feet long, I believe, and the pressure chamber is 10 feet in diameter, which figures out to be a volume of roughly 600 cubic feet per man in the six-man crew.

At the lower sector there you see the housing for batteries, which was non-pressurized. You see four Vernier-type motors to provide stability and emergency propulsion. The motors are rated at, I believe, 25 H.P. apiece, and they can rotate in various direction.

You can see along the side of the craft the portholes for observation.

You might ask at this point why was NASA involved in this type of mission. And there are certain similarities to this type of mission in space. The one that is most obvious is the long duration. Another, less obvious, is the make-up of the crew has certain similarities, in that they both have scientific and operations goals. They were stuck in a confined environment for very long duration, and it was isolated, and there was a certain degree of hazard. Now if these kind of parameters can reveal problems, perhaps they can reveal those similar to what you might encounter in space.

The ones that were most obvious to address were the habitability problems. Those involved how one might utilize space, how one might handle clothing, how onemight shower and keep clean. And then there was a maintenance aspect that we looked at. Chet will go into a lot more detail on the specifics.

The question that might also arise is why not check these things out in space itself; and I think very simply the answer to that is the cost. If we could get answers to the questions without going into space at a magnitude less cost, it would be very worthwhile.

Then there's the final note I would like to end on, and that is: perhaps we at NASA should look more closely at the space developed technology, especially with regard to its applications in the ocean. If we're developing similar technologies, perhaps we could make use of them in both environments.

And with that brief introduction I'll introduce you to Dr. Jacques Picard, who is responsible for the mission to a large degree, and who is going to tell us about some of his subjective comments regarding the mission itself.

DR. PICARD: Gentlemen. If we consider the purpose of our mission was to stay one month under water and keep in good condition, and possibly to make some interesting oceanographic and other observations, we may consider that our mission was completely and entirely successful.

However I believe that we do not gain anything by repeating it was a complete success, and as we plan toward other missions, and especially as we plan to use this boat or a similar boat for other long duration missions, I believe that we will never make any progress if we do not look very carefully into the things which were not very perfect, let's say like this. It may -- and it was, only very minor things which were not good. But these minor things may turn to be very important in other missions.

So I do not intend to criticize anybody by speaking to you today only of things which I believe were not good; I just believe that it is a more constructive way to do. And if we face really the things which were not good, we may find automatically away to improve other missions.

So there will be mainly six kind of chapters, or paragraphs, that I would like to discuss with you on which I believe several points can be improved.

The first point was the so-called life supply. We stayed for one month, and we could live for one month, but it was not always perfect, and we didn't feel always, I believe, as well as we could have.

The first things which happen if we are closed in a relatively small room -- I should say a room in which nobody is smoking; otherwise it would be just about the same as here -- the producing of CO_2. And we know that by breathing we produce CO_2, and the CO_2 will be more dangerous and more immediately poisoning people than the absence, or the decreasing amount of oxygen.

We first said that we would never have the CO_2 level higher than 1 percent; which was the figure that I recommended myself. And after this, after some investigation, people believed that we could go up to 1.5 percent. I don't believe it was a good idea; because obviously CO_2 is a poison, and the less we have CO_2 in the atmosphere the better it is, even if we don't feel directly the inconvenient.

And it's interesting to know that it doesn't take more lithium hydroxide -- because this was the product that we used to absorb the CO_2 -- to keep the level at 1 percent than at 1.5 percent, except for the very few beginning -- the very first hours. As a matter of fact, as we never have been more than 1.5 percent, it means that we had enough lithium hydroxide on board to absorb all the production of CO_2. So we could have worked a system which would have been more efficient in order to absorb more CO_2 and keep the level at just 1 percent.

The system that we had basically was liquid oxygen which was evaporating slowly inside of the hull, and we had a completely passive system for absorbing the CO_2, just twelve panels of lithium hydroxide fixed on the wall of the hull, and just by the normal moving of the air the CO_2 was absorbed. This had some advantage, especially because liquid oxygen is a good way to save weight. It takes for the same amount of oxygen less total weight than if you used compressed oxygen.

However I would like to point out a system that we used 20 or 25 years ago for the first bathyscape, especially for the TRIESTE, when we started with the TRIESTE. We had three containers, one containing lithium hydroxide, another one containing silica gel, and the third one containing activated charcoal for odor absorption. And the oxygen was not liquid but was in compressed cylinders. And when the oxygen got out of the bottle it sucked part of the ambient air and blew this air through these successive three tanks with lithium hydroxide, silica gel and activated charcoal. So with the same system, without any power -- because the power was requested before the expedition when we filled the compressed oxygen in the bottle -- without any power we had a complete automatic system which absorbed extremely well and which kept the level of CO_2 at a lower degree than what we had on the BEN FRANKLIN.

So this could be made, in some case -- I will not discuss it: it was a case of doing enough with the BENJAMIN FRANKLIN, but it is a possibility, at least.

The second problem that we had was CO. You know that carbon monoxide is a real bad poison, much stronger, of course, than carbon dioxide. And we had some on board, and we don't know yet -- as much as I know myself-- exactly where it was coming from. We have some reports according to which some CO is produced by breathing at the same time as CO_2 but at a much lower degree, but there is some. And some other people, some doctors told me it is not true. As a matter of fact, the CO cannot come from the human body, but could come possibly from some evaporation of heated plastic. Some plastics, when you heat them, produce some CO. So we could imagine that we had CO produced by insulation of electric cable, for instance.

We had onboard a system to absorb the CO -- more exactly, to change -- to burn, chemically speaking, the CO into CO_2, but this system did not work on board, probably due to the humidity of the air, of the ambient air.

It was also a little bit uncomfortable on board psychologically speaking, because although Grumman knew the limit of the CO that we could have on board, we on board did not know exactly up to what point we could go. And speaking with the surface, we had theimpression that we could not go over 25 parts per million, and later on it happened it was 40 or 50 parts per million. But this created during the mission some kind of uncertitude which was uncomfortable.

The question of the humidity is the same as the one of the CO_2. We have never been more than about 75 or 80 percent of percentage of humidity in the air. So it also showed the silica gel that we had was sufficient to absorb all the humidity produced by the six crew members and the other equipment -- the kitchen, the shower, the toilets, and so on. But, again, if we could keep a level of 75 percent with the same amount of silica gel, if it is well used, with better efficiency, we could keep the level much lower, maybe 50 or 55, which would have been more comfortable and which would, maybe, have allowed the CO equipment burnup to work.

The temperature on board was not agreeable. It was relatively good when we were at a normal depth for the drift, which was six to seven hundred feet, although it was never warm enough to really be absolutely comfortable. And we knew in advance that the Gulf Stream was a so-called "warm" current, but "warm current" doesn't mean anything; it just means it is warmer than the surrounding water. And especially when we made relatively deep dives or bottom excursions, then we arrived in water which was 50 or 54°F. which gradually lowered the temperature in the boat also to these kind of temperature, and it was really uncomfortable and cool. And two or three times we had to short the deep dive just due to the fact it was so cold we could not practically stand it; at least we could not stand it and work at the same time.

So we need a better insulation. We had no insulation at all, as a matter of fact. We could insulate the hull. We need better clothing.

The clothing that we had were not good, not convenient, not agreeable, not warm enough, and irritating the skin for some crew members, but not for all. I was apparently more sensible than the average of the other ones, but I was facing the dilemma of having the skin irritated continuously or being dead cold, which was not very comfortable.

Then the food. We, of course, made a lot of jokes about the food. The main idea that we had-- You know, I am speaking absolutely seriously: we say everything which NASA is doing is perfect, so we'll take the food from NASA and we expect to have the perfect food. Apparentlywe didn't take exactly the food of NASA because it was not perfect.

(Laughter)

And it was extremely monotonous. And I don't know for what reason, but I believe it has been considered much more complicated than it really was. We had, for instance, some kind of cookies which were good because they were the only hard things to chew. For one month you like to have something hard to eat, you know, besides the chicken sauce and the beef stew and these very soft materials. So we had to use some of these cookies. But all the cookies we had were the same kind. And a rough analysis of these showed that about 30 percent was cereal, which was good, and roughly 70 percent was dust, which made it very, very untasty for the most, at least. And we just didn't know why we hadonly one kind, and why we had to make so much story for cookies, because you can go into any drug store in the States and buy a hundred kinds of cookies that you can keep for months and months, and they are very good provided you keep them in a closed box. So this is typically a case in which we looked much too far away when it would be so simple just to ask our wives to get us very good cookies.

(Laughter)

Incidentally, we had not enough tea or coffee on board. This is interesting because apparently when we are closed for one month together we have more opportunity to drink coffee, and the statistics which were -- I don't know -- provided by the Food and Drug Administration or some official office like this, were not concerning our case.

Mainly we had a problem with the water, also. We had two different supplies of water. We had one supply for the cold water and one supply for the warm water. The idea was that we had not enough power, or we didn't like to use our energy, battery power, for heating water, so we had four tanks very well insulated -- the cryogenic system, you know -- and in advance we had about 250 hot water gallons, and this water was supposed to stay warm for the full mission. It happaned for very good reason that two or three of these tanks didn't work, and the company that was supposed to repair them was out of business, and so on, and when we left we knew that at least two tanks -- and it happened to be three tanks, would not be very good. The fourth one was good and kept the water long enough. And also we had been very lucky with the power; we didn't use more power; we didn't lose any part of the battery power that we expected maybe it would happen; so we could use some of our power to re-heat part of the water. But we definitely were out of a good supply of hot water, which was also uncomfortable, especially due to the cold atmosphere in which we were.

The cold water was -- we had enough at the beginning but it happened that very soon Chet-- Let me say, before I accuse Chet: in order to keep the water drinkable it was -- we put some iodine in this water. And I don't know exactly why, but the result was absolutely awful, and it was practically impossible to drink this water.

I know that in the LM they had several experience with iodine and with chlorine. I believe that you found out that chlorine is better. We used iodine for other very good reasons. But the result was that that water was extremely bad, and for a few days at the beginning we were supposed to drink that water if we would like to have cold water.

And then Chet May made us a very good help when -- you know, he was in charge of looking for bacterias and viruses and those kind of bugs on board, and by chance he found very bad bugs in the water which had the iodine. So we said an order of not drinking that water. So he saved us really with his bugs.

But, of course, it was rather strange to note that the cold water with iodine got bugs and we could not eat it, and the hot water without any kind of disinfectant product -- iodine or chlorine-- remained absolutely good for the full mission.

One point: you know, at the beginning this water had been brought to very high temperature, so every bugs were killed. So this was good. But they didn't appear later on, even when the water got cold again. So this is something to think which could be certainly improved. Because if you have bad food, it's one thing, but you need at least good tea or good coffee, or good water in some cases. This I feel is a very important point.

We had a minor problem, which was mainly more accidental than the question of organization, with the toilet system. We had-- Everything was kept on board because we didn't like to pollute the sea, especially because we are drifting with the water, so we stayed with the same water, so we could not throw anything away. So we had waste tanks to keep everything on board which was disinfected and chemically treated, and so on. In spite of this-- Well, let me say that for the first three weeks, or two and a half weeks, I believe, it worked very well. And for the last one week or last ten days about we started to have some problem, some odors and so on, which were not good. And it is a pity, because we could have -- very easy we could have some, maybe fifty pounds of extra activated charcoal which would have solved the problem completely. But when some of us requested in advance to have an excess amount of charcoal for this possible purpose we have been told that everything was so fine and so well prepared that we would not need for any extra charcoal. And this was a mistake again, because the charcoal happened to work very, very well. But, of course, we need a sufficient amount for this.

Then if we continue: No. 2 that I have to comment is life conditions on board. Maybe I was a little bit unrealistic in advance, but I expected that the fact of staying six people close together for one month underwater would provide everybody absolutely a remarkable opportunity to work peacefully, to a -- I wouldn't say on a philosophical way of thinking, but a little bit like this, and to really be able to enjoy the trip and to have enough opportunity to think toward this problem and to work in a different way than what we do in our laboratories and business and so on, when we are continuously disturbed by the modern activity, let's say like this. And it was not the case at all, except for one occasion that I will tell you later on.

The boat was too noisy, much too noisy. We believed in advance-- Of course, you have to realize that this is mainly -- I am speaking to you now, so this is my opinion, what I am thinking now, and it may not be the same, for instance, as Chet May. Maybe he wouldn't say like this. But for me the boat was too noisy. We had, for instance, taped music on board. We had about twenty cassettes. And we had every kind of music from Mozart, Rossini, to the

Beatles, you know. And as I said, we had twenty cassettes. That's a lot of choice. And the first day we had a lot of choice. And the second day we still had choice. And the third day we had not much choice. And for the twenty-seven other days it was always the same music; which was very annoying for people who did not like the kind of music that the other one liked at that time.

And we had one earphone, for instance, one pair of earphones which was of extremely good quality, and when you got the chance to have the earphone you could enjoy the music very well. But we had one pair of earphones for six people, which was not enough, of course. And very often this music was just terribly annoying and noisy and preventing you or to work or to think or to sleep; which was important.

So this is very easy for another mission. We can have more choice of music if we like, but mainly we should have six pair of earphones and several places in the boat where you can plug, or in plug when you like, so you can even work on your porthole and have your music, the Beatles, and so, if you like them. So this should be done.

Besides this it was always too much noise because -- I believe this is everybody's responsibility. We had always to have two people being awake for the control of the boat itself. So of course we had people sleeping while people were working and others were having their lunch, and so on. So we had a little bit difficulty, and we should have been better prepared for this really to keep quiet while other people were sleeping, or trying to sleep.

VOICE: Was there any isolation?

DR. PICARD: What did you say?

VOICE: Was there any isolation?

DR. PICARD: No. It was practically no isolation. We had six bunks. The idea was that everybody could have one bunk for himself and relax there completely. And we had only a small curtain about the same thickness of this one here, which let the light go through: much too much, it was not dark enough in your bunks. And it didn't prevent any noise at all. So this is something that could be improved.

The best thing in this deal -- maybe I'm going too far away, but this is a goal I would like to reach for another mission of this: everybody should have one small room, completely insulated, with one porthole for himself at least, with one or two searchlights for outside, and a bed, a little table, and a shelf for placing his books and so on. And then he could isolate him completely and work in complete peace.

I lost many, many hours because I just could not work due to the noise that everybody was doing.

VOICE: That's the same experience they had down in Antarctica. They found that they've got to give each man a room there.

DR. PICARD: So the noise, this is very important.

?? Of course in our case, in the mezzascarp as it is now, we don't have enough place for making this. We have ten feet in diameter. If we would put twelve feet in diameter it would be very easy to do it. We could just do it very well.

So this I consider is an important thing in order to give to everybody the possibility of working well.

Myself, I happened to be awaked, among others, on the morning until early afternoon. And during that time the man who was in charge with me to be awake happened to be an extremely silent man. He never had any kind of noise. He was working always like this, and he never opened his mouth. He was a perfect companion for this. And on the morning I could really work very well because everybody was sleeping usually except of his one. So this was for me a very good time. But on the afternoon when everybody started to get up, the people who worked during the night, then the music and noise, and so on, started to come, and it was impossible to work.

Another thing interesting for the general condition of life: we had installed by NASA three automatic cameras on board, and these cameras were taking a picture every two minutes. So altogether every two minutes we had three pictures done. And the responsible people at NASA told us that if we didn't like we just allowed to stop the camera absolutely any time. All six people in the crew were allowed to stop the cameras if they liked. Nobody did it. And I didn't, of course, because the purpose was to -- part of the purpose was to take these pictures; so it would make no sense to stop the camera while they were installed.

But for me they were extremely uncomfortable. I really hated for one month to have these pictures taken every two minutes, and know that whatever you do you have this picture continuously, except for a small part in the middle of the boat which was kept for complete privacy.

I know that some other men -- most of the other people didn't care really for this. I had been told that I may be disturbed with the picture, with the camera for the first hours or days and I will forget them. I did never forget them. So it was very uncomfortable. But, again, it was part of the goal, so it's not very important. I'd just like to point out the fact that I did not get used to this camera after one month.

We had also below our bunks, installed by NASA also, we had some meter to know exactly how many hours we spent every day on our beds. This was good. This did not disturb at all. But it's a little bit difficult to interpret them and to understand the result. Because two members of the crew, Chet May and myself, for instance, we had portholes on our beds just close to the pillow -- which, by the way, was really wonderful in some cases. And very often I was laying on my bed just for looking through the porthole, because it was part of my job, you know. So if you just meter the number of hours I spent on my bed you will say "This man was sleeping all the time." -- which is not exactly true.

(Laughter)

No. 3 concerns the relations between the various crew members. For the newspaper mainly I believe ithas been said that we started with six men and we ended the mission as six friends. It is very nicely said. And I agree in one sense, of course. But it was not true, really.

We didn't hate ourselves at all. We had no major problem. But we didn't improve -- I don't believe, except maybe in one special case, we did not improve any kind of friendship during the mission.

I believe that-- We had some trouble. Maybe we were completely prepared to accept everything, and this makes it, of course, easy, because we accept everything. But it also maybe gave to some of the crew members the idea they were free to do anything they liked. And you know for one month if once somebody tells you "Don't use the light now, it disturbs me, or it takes too much power," and so on, once it's all right. But if it comes too often, and if somebody tell you "Don't use the light for the outside," and in the meantime he never turns off the light of his bed even when he's not there, you know, it becomes a problem. And especially because the heirarchy on board was not exactly established.

We had basically three chiefs. We had first the captain on board. The captain was responsible for the boat, so he was -- in one sense he was the chief of the operation, of course. We had the surface -- the surface was also what you would call the ground, which was also -- who had also some very important responsibilities. And finally one of the men was called the mission leader. So who was in charge of the boat, really? Who had the right to say "Now we don't use any more light because we don't have enough power,"or "we don't use any more hot water for a few days, just to save it", or "we will not make this experiment but we will make another one," and so on.

This was not decided in advance; and this was a mistake.

And if it was not adrama for our case it is due to the fact that we all had very, very high motivation, and we all were absolutely willing to stay one month under water. But this is just because it was the first time, because it was a quite special experiment.

And I, for instance, I was in advance decided to be never excited, neither outside nor inside for myself, and to accept absolutely everything. Because I'd just like to make the mission, I'd like to get some precise information in which I was interested, and I knew that by starting a fight for prestige and things like this would be ridiculous in this case. But for another mission I believe it is extremely important to establish exactly who is responsible for one thing and who is responsible for another thing.

It may be there is nobody absolutely responsible for everything at the same time. But at least for the various parts it should be decided much more than it was.

The next point, which is really just about the same idea, was the relationship between the crew as a whole and the surface. Again this was not very clear. And again this created some problems.

We had the impression -- mainly I had the impression that we had been treated on the water a little bit like children at school, you know; which was good, because -- which was not good, but understandable, because at school the management of the school, or the teacher, feels responsible for the little children and they say "You do this, and you do this," not because they like to have a law but just because a consideration is safer for the children: "Be careful when you get out in the street, and don't run if it is a car," and things like this. It was a little bit the same case for ourselves.

A lot of things that we could have decided ourselves was just decided by the surface; not a lot of things, but several things which happened to be relatively important.

For instance, once we had to ride to the surface because we happened to be pushed out of the Stream and our own power was not sufficient for going back in the Stream itself, so we had to go to the surface and be towed by the boat -- we didn't open the hatch, of course -- be towed by the surface boat and start a dive again. And for starting the dive we had the use of some ballast and we requested a special amount of ballast that we computed very carefully: we had full knowledge of the temperature, the density of the water, the density of the boat itself, the amount of ballast-- which was iron shot in this case -- that we had used up to now, and so on; so we requested to have a certain amount of ballast. And the man on the surface did not know the problem as we knew, of course, he had not all the information we had accumulated during the first ten days of the mission, but he was extremely careful and extremely willing to do the best that he could, and he just decided to double the amount of ballast that we requested. And we told him no. We needed about a thousand pounds, I believe, and he put two thousand pounds of ballast.

And at that time I would have -- or I would recommend for another time that the surface would discuss with us and tell "Well you believe a thousand. How did you achieve your calculations? Why do you say a thousand and not twelve hundred or eight hundred?" And on my side. The surface could say "I would recommend to be more because maybe darkness will come and maybe it will be night, and maybe the sea will be rough, and so we would like to be sure that you really start to dive very fast," and so on. Not a word of this. Just two thousand pounds, or whatever it was, without any comments. He told us how much he gave, so we knew. So as soon as we start to dive we start to drop the ballast, of course.

But psychologically it was not good.

Another point, for instance, in which we had been not treated as I would recommend for another mission: The mission was thirty days, and we start the dive in Palm Beach on July 14th at about eight o'clock P.M. So the dive was supposed to be finished at eight o'clock on July 13th, obviously. And the surface decided that we would stay in the water until August 14th, a half a day more, in order to make the dive -- to end the dive in the morningand not in the night.

By the way, I happened to be of the same idea, especially because the weather was not quite good. It was much safer and better to stay half a day longer. But, again, the surface should have discussed with us and explained to us, saying "Listen, you know the sea is rough, and what do you think yourself? Don't you believe it would be better to stay twelve more hours? You have been 720 hours, can you stay 12 more hours?" And we would have said yes, no problem, of course.

A few people who were more used about the sea than some others didn't see the importance of getting out of the boat in the morning. They were used to the rough sea and didn't care about this. So they didn't understand why it was better to stay twelve more hours. They were very angry. And this was absolutely useless. I know they would have accepted the idea of staying half a day more if it had been discussed in advance, just for some -- to be nice, to be a little bit more psychologist, maybe; not just to apply a precise rule in this case.

Another thing which also was not perfect: We had to fill -- for the psychiatrists, the doctors who would like to know exactly what we were thinking, and so on, now was our own personal evolution and feeling during the mission -- we had to fill every day some question, sometimes two pages, three or four pages ofinformation, and so on. And in advance we received a letter from Grumman telling that all this information will be absolutely secret and will not be published for any reason. So we accept to fill this, which was a little bit confidential, as you may understand, of course. And once suddenly the surface tells us to -- ask us to give some information coming from these sheets. And at that time we should have taken some sheets and look and speak by phone where everybody could hear and give some of this information. And two or three of the crew members refused to do it. They said "No. We received precise information -- precise instruction in a letter from Grumman and NASA telling it would be absolutely confidential, so we refuse to give it."

And the captain was quite embarrassed, because he had received the order from the surface to give this information. And so he told to the surface -- which happened just to be, of course, obviously in perfect good faith: the surface man who was interested in this did not know that it was supposed to be confidential. And they did not insist. They said "Well, if you don't like to give it, just keep them."

But in the meantime the captain has said to the surface "I would not like to give it to you, but if you insist of course I will do it." And so this was a very, very bad problem. Because again it showed the problem of the hierarchy: was the captain of the boat allowed to, or had to give information to the surface when he knew, or when everybody knew that it was secret, just because the surface man did not know it was secret?

So again the question of the dependence -- the interdependence of the various people was an important problem.

The last point that I would like to discuss with you a little bit only is the criterion of abortion. In what case had we to abort the mission? And for me, as much as I was concerned for thirty days about, or at least, let's say, twenty-seven days, I had continuously the feeling that we would have to abort the mission, that something was not possibly going good, and that we may be in minor trouble which wouldhave obliged us to abort the mission. And this is because some facts happened which were not precisely enough decided in advance.

For instance, one of these things was: inside the boat we generate gradually a little over-pressure, not due to the life supply system but due to a small leak that we had in one valve, and we didn't use this valve usually, but occasionally we had to use this valve, and every time we used this valve we had some leak inside the boat and the pressure built up gradually. And we did not know how far we could go with the pressure without having to come to the surface and ventilate the boat, or at least equalize the pressure. And the fact that we did not know it made the thing also a little bit uncomfortable. The same as I mentioned already for the CO, we didn't know exactly how much -- how many parts per million of the CO we could afford to keep without any damage, although it happened later that Grumman itself knew it very well.

One thing also: we didn't know clearly what we wouldhave to do if some part of the scientific equipment would not work. And this had been decided in advance, but we knew that it could never be really applied as it was decided: it was probably too severe in advance. And it was too severe mainly due to the fact that as a matter of fact Grumman -- and, at the same time, NASA -- had one goal to achieve, and the Navy another goal to achieve. And both goals were, technically speaking, completely different. The Navy was not interested in staying thirty days under the water. For the Navy, we could have come ten times to the surface and opened, ventilated the boat, and go down again; because to the Navy it would have been exactly the same because the scientific, or the oceanographic data collected like this would have been practically exactly the same. And NASA and Grumman -- and myself, too, by the way -- were interested mainly -- not mainly, but widely, let's say like this, in staying in a closed boat drifting continuously below the water for one month.

So the fact that we had two different goals to achieve, it produced during the dive some kind of uncertitude which was also -- which could have created some difficulty.

And besides this, we had a very good dive for one month.

So that's all I have to tell you today.

(Applause)

VOICE: Thank you very much, Jacques.

Are there any questions regarding Dr. Picard's presentation?

VOICE: What was the pressure?

DR. PICARD: Atmospheric pressure.

VOICE: Did the cameras make noise when they took the pictures?

DR. PICARD: Yes, but the noise didn't disturb me at all. We could hear it continuously, but I was not disturbed by the noise. I was disturbed by the idea.

VOICE: Could you have stayed down another fourteen days under those conditions?

DR. PICARD: Practically we would have had some difficulty due to the toilet system, which started to give some trouble. The toilet started to give off odors; not too bad, it was mainly chemical, but it was uncomfortable. We would have been in a little bit of trouble really.

If we would like to, with the same boat, to extend the mission up to six weeks, or even two months, we could do it. And if we would have had to stay a few more days I believe we could have done that very well.

Psychologically, yes. But on one condition, you know: decided in the boat, together. What shall we do? Do we accept we stay a few more days? And we could have done it.

But several people, two or three people on board would certainly not accept the decision (inaudible)

VOICE: I understand it's possible to modify the boat now. Could you actually add more power to it to get a more reasonable power level to work with? --with modification?

DR. PICARD: We could. The next step would be to use fuel cells. Fuel cells for the amount of power that we need are awfully expensive.

(Inaudible)

Of course if we have atomic energy then we could do it.

VOICE: In terms of the supplemental data capabilities you had, such as your own logs, films, etcetera, did you significantly add to the Grumman data spontaneously? The things you took, the pictures you took, did they add anything that they --

DR. PICARD: I don't understand the question.

VOICE: You had cameras on board which you were free to photograph anything about your own activities.

DR. PICARD: Yes.

VOICE: Did they find anything valuable from these films?

DR. PICARD: No. We didn't take any good pictures. We had a few relatively good ones, but nothing really good; and for one major reason: in the hold there are batteries, which is in the keel under water, which worked extremely well during the full mission. We had some trouble with the battery before in a preliminary test dive. And we were always concerned to lose part of the battery.

In other words, we decided to keep the battery as much as possible just in case if we lose some part we still have enough for the life supply and so on.

So in several cases we could have taken very, very good pictures and movies, and we just had to renounce them, again because the main purpose was not to do pictures under water but to survive for one month. And just for safety we didn't take that.

I think all the pictures that we did -- that everybody in the crew did, have been given to Grumman.

VOICE: Let's cut off the questioning at this time so we can continue.

DR. PICARD: One thing I forgot: we could use a silver __________ battery. But 28 tons of silver__________ battery would be more expensive than the rest of the boat.

VOICE: I know there has been talk about modifying the PA-15 and making it bigger and adding other features like _______________ andso forth. I was wondering whether there was consideration being given to the power. I assume that there has not been.

(Simultaneous discussion)

VOICE: Well let's continue the presentation,--

VOICE: I think we can cover that power situation later on in the discussion. Let's continue on with Chet. Time is running out.

VOICE: Chet May participated in this mission trying to get quantitative data for NASA, and he did it in several areas. And that is the gist of his presentation today.

MR. MAY: What I'll talk about today won't deal with the Navy work but it will deal with the NASA program that we -- when we took a look at it, without going into it again, the justifications of the commonalities between underwater systems and space systems and the potential cross-over where there are common areas of study, and then in turn where you can use these kind of systems as an analog. I will comment briefly on these. And, again, the kind of rationale it would take to go through this, I can go through if you want. But I don't think right at this point the time would merit it. We can discuss it later on after this mission.

The NASA program, our objective, our over-all objective was to investigate the feasibility of utilizing underwater systems, in this case a mobile underwater system, as an analog for a space station in certain areas: in the living and working area, and as a test bed for hardware.

In looking at the BEN FRANKLIN and the Gulf Stream Drift Mission we found mostly that the commonalities were in the living and working area. Our approach in this particular program was to go through to find the areas of similarity between the ocean and space, and then to try to define some sort of a study for the Gulf Stream Drift Mission which made sense that we could both obtain quantitative and qualitative type data from.

We have done this, and out of this, then, propose any programs that we feel in the future could be -- could for NASA provide data which is needed.

So if we look at the areas that we chose for the Gulf Stream Drift Mission, the hardware was not similar, so obviously it was the living and working area and activities that we were concerned with. Out of this we developed a program in the psychological and physiological area -- but the physiological isn't noted here: it was left off by the guy fixing the chart.

We looked at habitability for living and working conditions with respect to these kind of characteristics. We looked at the system, since it was a completely closed environment for thirty days: nothing going into or out of the system, including waste: we looked at the microbial aspects of the mission. And then we have a lot of data with respect to maintenance in terms of the effects of weightlessness on maintenance, and in terms of the space suit and how it affects maintenance, but we haven't any data relative to confined environments of this nature on how to -- is there a delta here or some sort of an effect that this environment itself offers on the actual performance of maintenance tasks during an actual operational mission.

So it was our intent here to identify the tasks and try to see if in the actual performance of the maintenance tasks there was some sort of delta in this area.

I might add -- and I will talk about it later on as I go through this area -- that the only kind of maintenance that we actually had to perform on the mission were scheduled and non-scheduled tasks. None dealt with the safety of the crew. And then we had an area where we looked at the mission control and problems.

Now to go through these particular areas section-by-section and tell you, or show you some of the quantitative data that we got out of it: of course we have quite a few subjective and picture presentations we could show, but this is mostly to try to give you an idea of the kind of data that we collected in each one of these areas.

We gave a full day-long presentation at Marshall about two or three weeks ago, and what you will see here today is a summary of that presentation, to try to show you in essence some of the trends and some of the ways in which we treated the data.

Obviously when you go through these sort of studies and you take your first cut through it, there are other relations and other ways in which you think the data could be treated that make sense. We have seen some of these already, but we haven't had time to really go into them. What I will show you will be sort of representative of the way we treated the data to date.

Now in the life science area we had the objective to identify the crew reactions and measure their performance. And we did this with interviews, tests, diaries, logs, voice tapes, time lapse photography, and psycho-motor performance measurement. And there is another measurement device on here which I will show you some data on, but it's not on here, is the sleep monitoring device which we used

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from Dr. deLukey and Dr. Frost at MSC and Baylor University.

I would like to say, make a general comment with respect to the life sciences area, and that is that as I think Dr. Picard pointed out, there are different objectives on the mission, and obviously the Navy's objective was not one to meet the NASA problems, or to solve the NASA problems.

They did agree to fill out the logs. You can question the confidence relative to what these logs reveal in terms of when the logs were filled out as opposed to when the problems actually occurred. If you are not, as I have-- And this is my own interpretation and my own observation on the mission -- not specifically concerned with those problems when you come to those logs to fill them out, many times the questions are repetitive and you just put down an answer to get it off your back and get the log in. Well, I mention this to show you that in one case, in my own case, I was, of course, very concerned with the particular data that we got. And so I took a considerable amount of time to answer the kinds of questions.

Now I'll show you data -- and I don't mind. in my own case the data is confidential, it's confidential but it's also based on the individual's willingness to reveal what his comments were. In my own case I feel that the comments and the things that I revealed can do us more good by throwing them out on the floor and kicking them around. So the kind of data that I will show you will be, some of the data will be -- particularly my data, and also will show typical data from some of the other guys. And I'll also reiterate that I'm not sure -- it doesn't mean, I don't think, that maybe the stress throughout the other crew members wasn't as high or peaks, and we couldn't measure it. Maybe the instruments that we used weren't sensitive enough to pick up in terms of the involvement or the revealing aspect of the mission.

I'll go through the sleep monitoring equipment.

We had this particular instrument on board. It had seven sensors on the head which monitored four phases of sleep.

Now this chart is a little busy, but all I want to show here, is to make a point with respect to how this kind of data is analyzed.

This is a day which was in the latter part of the mission, and it's eight hours: two and a half hours on this line, two and a half hours on this one, and two and half on this-- Well, anyway, it comes out eight hours.

(Laughter)

That doesn't quite come out eight hours, but this one comes out eight hours.

But you have four stages of sleep. And the REM, which is the rapid eye movement, gives you an indication of when the subject is dreaming. Now in this particular case you can see that it was better than an hour-- Normally what happens here in this environment, when you and I are at home in our beds asleep you go through these four phases of sleep in 90-minute cycles. You go all the way through Phase 1, Phase 2, Phase 3, Phase 4, into the Phase 4 deep sleep. You spend some time in each one of these phases, depending on whether you have something bothering you psychologically or whether you've got problems or not. And then you come back to your dream -- back up to the top, to your dream period. You gothrough a dream period, then you go back through your four stages of sleep and back to your dream cycle.

Now this particular chart,--obviously you see that didn't occur. This was later on in the mission. It took some better than an hour just to get to Stage 2. It took even more to get to Stage 3. Stage 4 was not reacheduntil better than three hours into the sleep cycle. Mostly this shows that on this particular day the sleep was sort of -- you went through a drowsy state and did not sleep well, the subject did not, on this particular night.

Now this is-- As you will see in the data, this is typical. I picked out a couple of graphs. Now there's a detailed presentation in this particular area in itself which Dr. deLukey has put together, which I think is very fine, because he has had Baylor University running these things through the computer to quantize his results. But I have got a couple of charts to show basically some of the ways he has treated the data.

This particular scale here is in minutes. This data here is the baseline data pre-mission: two points was where the subject was in Houston in the Baylor University taking his sleep. One was where he was at West Palm Beach.

Then these are days in the mission. This is Day 1, Day 2, Day 3, Day 4, Day 5, Day 6, Day 10, Day 14, 17, 21 and 23. We also took data in the last three or four days of the mission. However, some way the data didn't come out on the tape. So we lost about three days of that. But you can get an idea of the trend.

Obviously there was apprehension here with the pre-mission data relative to -- we were six weeks late in getting started on the mission. There was apprehension really with respect to whether we even go on the mission or not. And Ithink some of the time in getting to sleep, in getting to Stage 2 -- I picked out Stage 2 and Stage 4 to show you -- it showed up.

Obviously when we went on the mission, the subject, because of the relief of the tension, the going on the mission, and this sort of thing, fell right into -- and the workload that he had went very fast and had very good sleep records with respect to his baseline, right up to about Day 12 or Day 13.

On Day 14 things started happening, and the time to get into Stage 2 started increasing. On Day 14, Day 17 and then Day 21, you can see it increased very significantly.

Now if you get this trend -- and of course the trend went right on -- could go right on like that.

If we look at Stage 4-- Now remember, this scale was in minutes. This scale here is a log scale in hours.

Again, you see that Stage 4 was attained sort of rapidly; stage 4 more rapidly at the first part of the mission. And then again started having problems in getting to Stage 4 to where on Day 17 it took better than seven hours to even get to Stage 4. But if you had a mean curve through this, the point is the trend is a very increasing type of a curve.

VOICE: Chet, can you relate, like on Day 17, to specific events?

MR. MAY: Yes. I can relate Day 17.

You'll see in this particular man's chart-- I will tell you one thing that occurred which probably was one reason that bothered this particular individual on Day 17 in this area, was that the crew had said around Day 13, 14, 15, in there somewhere, that it would be nice to get word from all of our families. So we sent a message to the topside to contact all our families and tell them hello, and try to get them to respond in some way back to us. Well for the whole crew, after about two days -- you know, this took time, when they got back to West Palm Beach and contacted the families -- for five members on the crew word came back relatively fast. One member of the crew -- in this case this particular member -- did not get any word, and when he called up to find out the reason, they didn't know what the reason was, but they would check it. Some thirty-six hours passed without any feedback to this individual, and when he called and asked again, and repeatedly, about four or five comments, they kept putting the thing off; they wouldn't give him -- they said no, they hadn't been able to reach them, they couldn't get in contact.

Well it came out there was probably some four or five days passed here before they got any word to the guy about his family, then they said it was okay. However after the guy got back he found out that really what had happened, his wife was in the hospital and there was no one at home. His wife spent twelve days in the hospital with an operation, an emergency operation, it occurred.

So that kind of thing-- But hot knowing I think had a lot to do with this particular individual in that stress. But it is representative from the standpoint of, there are things that will bother individuals, I think, in the mission as they occur. This is just one way of this particular individual being stressed.

If we look at another means on the mission that we tried to measure performance, we looked at the Langley research device. There is a Skal -- this has become known as the Skal box, or on our mission it became known as the NASA pinball machine. The hypothesis, of course, is that it does measure performance, and of course Dr. Skal at Langley

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is using these in quite a few studies. They were used in Tektite-1, and it's being used in several other studies, to try to baseline the piece of equipment to see, in effect, how it does measure performance.

I will say that in our mission-- Obviously the machine works. You have fifty problems which show up in these four sets of lights. You have a foot pedal for each foot and a hand pedal for each hand. Now when the problem shows up you've got to go through some sort of sequence to match these lights, and not until all four of the lights are matched does the problem go to the next problem. So it does in essence measure in your own mind whether something is bothering you or not. If something is bothering you you just sort of take this -- the time that it takes to work that problem increases. That at least is the theory.

Now in our particular-- Obviously you have to be off the learning curve with respect to this particular unit, or else -- before you start the mission, or else the measurements on it are non-interpretable.

We had three men on the mission which the data shows were off the learning curve prior to the mission. The psychologists had a contest prior to the mission, and three of the guys participated in that contest and had sufficiently worked the machine enough to where they were off the learning curve. So with respect to those three individuals-- We have gone ahead and plotted all six of them, but I want to show you a couple of curves with respect to two of those individuals.

This is the mean, where we look at this as being in time, and these are of course deviations from that mean. We look at this particular individual's curve, and we can say he was in good shape here, he had something bothering him -- if you accept that this machine does measure performance, he had something bothering him; he got okay here; he had something bothering him in here; okay in here, with another little perturbation. But toward the end of the mission the stress started staying above the line, primarily at least the last week of the mission. So that's not too unacceptable.

However, what happens to this particular individual with respect-- He was also off the learning curve, had a very consecutive average, and seemed to work pretty well. And again here was the same individual who had a stress period at the very middle of the mission with respect to the information -- the lack of information about his family. And again toward the-- after this he sort of felt relaxed and put it out of his mind, and we're right back into a very well -- and even channel fever didn't get to him here, at least from the standpoint of the operation of the Skal device.

Well we also had from our logs mood scales, psychological well being, depression and fear and these sort of things which I haven't shown you curves on. we have them. But I would like to show you in terms of one of the individuals, and, again, this is the individual that we did have -- felt we had good quantitative data as well as good responses in the logs.

This particular curve -- again, here are the number of meals -- I think this area was representative of the whole crew. In other words, as the meals -- in the beginning of the mission the eating of the meals together was more frequent than it was toward the end of the mission. Toward the end of the mission the eating of the meals alone for all of the -- at least four of the six crew -- we were in two -- three dyos -- we had broken up into three sets of two; where we ate our meals together. Obviously all six of us couldn't eat together in this vehicle. So we had broken up into three sets of twos to eat the meals with.

Now two of the sets, certainly the data showed a definite trend toward a separation toward the end of the mission, of eating more and more meals alone, significantly. One set did not. And, of course, we also have some theories as to why this occurred.

If we look at the psychological wellbeing, toward the middle of the mission -- it got pretty bad toward the middle of the mission. But if we look again at what happened in here it was at the same time of the family problem, and again at the same time the boat was out of the Gulf Stream and the potential problem of cancelling the mission existed very strongly. But toward the end of the mission, as can be seen, the trend started going up. And as I understand it, maybe this isn't unrealistic, it may be the trend in these kind of studies, when you start seeing that your psychological well being gets better as you see the end of the mission in sight.

Again the depression was the lowest at this point, and, again, that may be the trend that you would expect in this kind of condition.

In here the Langly scores show this big dip that occurred in the middle of the mission.

However I do feel I want to show you one representative curve of the depression and the psychological well being of one of the other individuals.

As you can see hre, in his data, from the logs, his psychological well being did not change significantly. It was still even on the down slope, though, toward the end of the mission. But the depression was, again, coming back up toward a zero point, or at least a nonchalant point.

Again, this particular curve may be more representative of the other crew members than the curve you saw before. I again say that I don't think -- I don't think it's the instruments -- I don't think it's the participation; I think maybe it's the convincing of the crew of the need of the data really to reveal themselves. Because I know that there were problems. I was there. I know there were problems. And I know that these problems didn't show up in the logs. And it is a case Ithink where individuals just hesitate to reveal how they really feel about a situation.

If we look at the results, then, from this particular area, we see that the mood charts and the psychomotor measurements we felt were insensitive to the mission events because, again, we had one data point that seemed to be -- we felt we had a lot of confidence in, but, again, we felt that if we can -- in future studies specifically, you need to do a lot to try to convince the crew to really reveal, and that the confidence that the data is going to be confidential, and to really reveal their true feelings and the way they really feel about the problem.... There's no way you can get this data mechanically. You can get through observations, position and time and location, where they spend their time, and that sort of thing. But to really know how a guy feels you have to have him reveal it to you himself.

I think that, in essence, is one of the problems. Of course the time and location data is something you can extrapolate from, and maybe yourself, or the psychologist can say -- put certain interpretations on it. Again I feel and think that in order to get to the real events on the mission you've got to convince the crew to reveal themselves.

Signs of depression did occur specifically in all areas during tow with respect to all of the individuals.

We did, again, accomplish the mission in spite of wide variations in background. I think what you can derive from this particular conclusion and recommendation is that maybe motivation ought to be the No. 1criteria you use in selecting crews. I'm not saying that you don't consider other kinds of things such as compatibility and other psychological measures. All I'm saying is that motivation ought to rank pretty high. Because we feel, particularly in this case, it was high with all of the individuals, and as a result the problems were minimized.

I'd like to get into some of the habitability problems we had.

Again, here we wanted to measurement environment from the standpoing of knowing what effect the contaminants in the environment had on the individuals; if so, if we could correlate this with -- when other problems cropped up, to be able to correlate this, if it was needed. And, again, how was the space utilized? What kind of food? How was the food, clothing, and this sort of thing.

We had time lapse cameras, we had counters, and light meters, noise, diaries, etcetera, to measure this.

And from all of this data, again, I will show you some of the representative kinds of data.

I also thought I would show you the layout so you'd know the interior aspects of the vehicle that we're dealing with.

This was our wardroom. We had seats around the side of the vehicle, which you'll see in some of the pictures.

This was the command and control panel on the port side. Our water tanks over the galley on the starboard side. One bunk on the port side right across from the galley.

We had the head on the port side, the shower on the starboard. We had two bunks on the port side in this area, one bunk on the starboard side. We had oceanographic and scientific equipment in here. And the aft hemisphere had a telescoping structure in it for an escape mechanism in case of emergency and was unusable in terms of habitability, we used it mostly for observation.

You can see here that we did have lights throughout the mission. We did have portholes to look out of and make observations. And these lights we used considerably throughout the mission.

VOICE: I only saw four bunks.

MR. MAY: There were six. THere was one in the galley -- across from the galley; three back where the plankton sampler was, and then two was over the scientific instrumentation. They folded up. That's the reason you didn't see those two. There were six bunks.

I want to just give you a little bit of what the boat looked like in the forward end. Of course this is where the table was, and many sessions was held, as you see, right here.

This was the silica gel, one of the lithium hydroxide panels. Here was our music recorder, and here was the set of headphones that Dr. Picard talked about.

This was the mid-section of the boat. You can see in here, here was all the bunks, and the blue curtains that were mentioned. Here starts the -- this way back starts the oceanographic equipment, and a lot of the work was done on the bunks in the mid-section. --a lot of my work was done on my bunk. I got nicknamed "The Bunkonaut" in the mission.

This is the -- you can see the rear aspects of the boat here. This is the telescoping structure that came down. This is Ken Haig trying to get in his bunk. This is the overhead bunk over his equipment. It did fold up in the daytime.

And you can see some of the storage problems that we had back here in the back, some of the problems we had in actually utilizing that particular area.

If we look at what the surface -- what the volume and this sort of thing was, we had 177 approximately square feet of surface area; we had 1372 cubic feet of volume; and equipment space, 381. We had approximately-- That comes out to be less than 30 square feet per man ifyou just look at total volume. That's not private area; that's total volume.

If we look then at the environment to see what were -- each one of these we have plots for, but I showed you the summary chart in essence to show you how these parameters came out. We had a variation in the pressure from 1.01 to 1.2. The temperature, 53 to 84° was the range. The average temperature came out to be probably around 66 or 67°. Because this temperature here only occurred in maybe seven or eight times during dives to the bottom.

The humidity varied from 63 to 83 percent, with the average being around 75 percent. The CO_2 -- we always changed the panels at 1.5 percent. The O_2 went up from 19 to 22. We did pick up methane in the boat, 190 parts per million. We had a gas chromatograph, we had 38 Dregger tubes which I used. We had syringes with which we took samples periodically throughout the mission and brought back for detailed laboratory analysis with a sophisticated gas chromatograph. And we also brought back the contamination removal cannisters that we had.

The CO got up to 40 parts per million. Actually my count in the logs was 44. This was the laboratory count. And, again, we were told we may have to abort the mission at 25 parts per million, but they moved it up to 50 parts per million. This was sort of an arbitrary thing it seemed to us. We weren't being bothered by it, so we went along with it.

The hydrogen, we got 420 parts per million. Ammonia, less than 1 part per million. And we picked up some ketone in the environment.

Now if we look at the way the cameras were located and the kind of position that we finally came out with, as Dr. Picard said we had a private zone in the boat which was from here to about right here, this area in here where the head and some of the bunks were was a private zone. We had three cameras, one located here looking at the front end of the environment, one located through this area picking up the information in the galley and the cockpit, the command and control panel. We had this camera looking forward which picked up the activity in the rear hemisphere and went through and picked up this area, and because of the wide angle of the lens got the activity in the scientific instrumentation area.

If we look at one of the ways that we used that data -- and there are other ways: I just wanted to show you-- We had planned time lines throughout the mission, and then we used the film to actually go through and determine how effectively the men, the various men kept to those time lines. All of this is in the final report; however I just wanted to show you one graph here which gave you the idea of locating guys in these areas. And when they were supposed to be in those area by the planned time lines is shown by the blue area, and when they were out of the area in some other area that they weren't supposed to be, according to the time line, is shown by the green area. So this guy followed it in certain aspects, and in other aspects he did not. And other guys, maybe on particular days, they followed it very thoroughly.

I was going through to try to give you some idea-- Habitability is a very difficult factor to try to get a feel for the parameters, as to how they really -- how you can really get a feel for what the problems are in habitability. And I've shown a Vu-graph here which kind of shows the complaints. Ithink that complaints are indicative of some of the habitability subsystems that are given to the men on the particular vehicle.

Over-all, the logs requested complaints at certain times throughout the mission. When those complaints were requested a high number of complaints were obtained. However even when the logs did not request complaints they were still given relative to certain aspects.

And I might say, to go in and see what this is made up of, what is this total complaint business made up of, I might say that in the logs these are some that we picked out with respect to the ones that were on this side were again selected or -- these were volunteered without the logs asking for them.

As you can see, the top complaint was communication with the topside, with food running a close second. Again, the furniture in the vehicle. The clothing. More furniture, bunks, temperature control, accessibility, water.

I might say that some of these complaints doesn't necessarily mean that the only time-- We solved, of course, the hot water problem with respect to the food by, as you will see maybe on one of the later charts, that the way we solved it was, when the water got down to around 165° we actually used boat power to bring the water back up. So the complaints in this area sort of got minimum.

We had no hot water for showers; we used only cold water.

Some of these complaints it was made known in the logs that they were going to complain one time about them and no matter how many more times you asked they said they were not going to complain any more; they were just going to make it known. So the numbers that you see may even be less indicative of the complaints that were there. In other words, when the complaints were made about the food and water they were sort of a relative thing. In terms of the food, I know in some of the logs the statement was made "I'll make this statement now and it'll pertain throughout the mission." It occurred around the fifth or sixth day. "And that is: every time I evaluate this food it's based on a scale of terrible. And we go from there with respect to food being fair, good, or poor, or this sort of thing."

So even though some of these comments were made, we still received complaints. And it was an upward trend with respect to the food. And, again, with the water, when you heat the water the complaints obviously drop off.

If we look again at the clothing, it was a continuously upward trend with respect to the cloth. Here again, two-piece clothing was some of the suggestions that would have corrected this. A change in terms of the underclothes every day. We had underclothes changing every three days. It was pretty bad by the time you got around to your changes. So you're talking about a laundry facility, or you're talking about at least underclothing changes at least daily, or something along these lines. The outer garment, if you'd change the design of it: if you're going to keep it one-piece, put a zipper in the tail, or something like that, and it may not be so bad. But these clothes, the material that they were made of, as Jacques said, did break some of us out in rashes, and this sort of thing.

The privacy -- again, one comment I want to make here: you must realize that the BEN FRANKLIN is a submersible vehicle. It was not designed to be a space station. The guys that were there using that vehicle were there using it as a submersible. The BEN FRANKLIN is a Cadillac in the submersible design area. It is probably a Model T, I hope, in the space station area.

So when you talk about complaints in terms of privacy, you've got to realize that these guys are used to going down in two-man capsules, staying eight hours all cooped up, with very little -- with just what food they take, and these sorts of things; no moving around or anything.

So with respect to privacy some of these individuals felt that the boat had lots of room, and this sort of thing, and it wasn't any problem. But, again, I reiterate, being there and knowing some of the problems that we have with the space station design, I can assure you that we have a long way to go with respect from the BEN FRANKLIN to make a space station which is habitable for scientists for the kind of time periods that we're talking about.

Was there a question?

VOICE: Would you elaborate on the time line complaints?

MR. MAY. The time line complaints?

VOICE: Yes. The voluntary complaints on the time lines.

MR. MAY: Oh. Well, some of the crew members didn't feel they wanted to even fill out a time line. They felt, some of the scientists felt that they wanted to go down, do their mission whenver they felt like doing it, and this sort of thing. And their complaint was, when we asked at the beginning of the mission, pre-mission, to fill out a time line, we almost had to do it ourselves. We had to go and sit down and talk with them, find out what their function was, periodically when they were going to do it in the mission, and try to come up with some sort of a time line for them.

I think based on this -- and I have dealt with time lines in space station work myself, I think basically what we really need is not a task-by-task type time lining laid out for the individuals, especially in space stations, but more by functions. If you've got a certain observation to make, give a guy a block of time to do that particular observation whenever it's needed in the mission. Let him do it in the daytime whenever he gets that time to do it.

I really don't think you need to go in and program every minute of his time. I think that was the big complaint, and I think it was a justified one.

Then in terms of what we're talking about, certainly in the contaminated area we had inadequate sensing and control techniques. Our taillight cannisters that we had did not control the CO or the odor or anything else. So certainly for this particular mission we had inadequate sensing and control techniques.

We had inadequate limits set with respect to aborting the mission. So you can certainly take recommendations from this.

I think, and I think the crew felt, that having real time data on these contaminants was important for the psychological well being in the mission as well as your own safety in the mission. If we go in space stations and we do not have equipment which can, in essence, tell you what that atmosphere is, I think it could be a source of psychological stress.

Again, we had a high level of complaints in these areas, and I can again go over specifically what trade-offs were made in each one of the areas, what the complaints were, but I think for the time that we have for this presentation it's difficult today.

VOICE: But as to the living and the working, Chet, I didn't understand that.

MR. MAY: Well, living and working complaints here mostly is, that in terms of living you normally think of your personal hygienes, your food, your recreation, these kinds of things, as being separate from when you go to do your scientific work. All of these were intermeshed together in this vehicle. I think what we really need to do is separate the living and working functions, and particularly have an area where you go to live and have an area where you go to work.

We look again, then, at the other aspect of the program, the third aspect, which is the microbiology study. I will go through this in terms of the data that we got and the objectives.

Again, we wanted to identify what the microbial growth was in the mission. We did this with these pieces of hardware. And what that data looks like in terms of the over-all profile of the mission was that in the -- I took three readings in the galley sink each day. I took three readings in the head sink and the shower sink every third day. And as you can see, in the first week of the mission we had positive readings in the endo and positive readings in the total.

Now if you say "What does that mean?" That means that our criteria here was zero reading with respect to endo, that is, in your home or anywhere else. That's the criteria that commercially is with endo, supposed to be with respect to endo, and I think some of the others. The guy that did this analysis has all the specs, the NASA specs and the commercial specs, and he could give you all the details. I don't have them.

In essence what I'm trying to show you in this chart is that of the sample period the dark ones of course are positive cultures, which means that as far as we were concerned the water was contaminated and it was not drinkable. We did use it for showers and we did use it for washing dishes and things like this. But we did not use it as intake to the body. And you can see that toward the end of the mission-- These particular dotted lines here was at times when I changed the micro filters in the water to try to clear up some of the contamination, but it did not work in each case.

By the end of the mission, as far as we were concerned, the whole cold water system was contaminated. We got all our cold water out of the cold water tank. We drained it out of the hot water tank and let it set and cool, and then we drank it and prepared our food with it.

The iodine, as Jacques pointed out, was a very crucial problem. Even 1 part per million of iodine you can taste, and it tastes pretty bad. And I tasted the water before I went on the mission, and I thought "Well I'll be able to drink the water, and I'll be able to prepare my food with it," but I think after you're down there two or three days it gets to you, and you don't really take that attitude.

The same way with the evaluation of the food. We had five menus. The menus looked terrific to me prior to the mission. And I even tried some of the food, and I thought

"Well, that won't be bad." But after you're down there for about three days, three, four or five days, it doesn't take very long for that food to get old very fast.

With respect to the surface, we can look here at the surface count in terms of organisms per square inch with respect to the actual rodak plates that I took throughout the mission, throughout the boat during the mission. And we had an Anderson air sampler that got the airborne particles, with this scale being the Anderson air sampler and this being the surface content.

You can see that pre-mission we were pretty dirty, the boat was pretty dirty, because we were loading it, and this sort of thing; and that's expected. But we washed the boat down and got it down to a reasonable level. However throughout the mission it started building back up.

At this point I read these particular rodak plates at 24 hours, 48 hours, and 72 hours, and at this point made a decision that we would wash the boat down. We had microguard, a special Microguard soap on board that we did this with. So we washed it down and brought the count down. But it came back up very rapidly. So we went into a different procedure, which is saying looking at the specific areas that were contaminated such as the galley, the head and the shower, which was the primary dirty areas. So we went in with a very high concentration in these areas, washed them on a daily basis, and we kept the contamination down to a very reasonable level.

Again here, this high count in terms of airborne was again when we had trouble with our head.

Now if we look at the body -- I took samples from seven parts of the body from every crew member every three days. And, again, this shows how that particular data came out.

From the standpoint of body simplification, in other words, if you've got fourteen different types of organisms, as we did, around when we started the mission, when we got to the end of the mission this had come down to the point of maybe nine or so. So what we're saying is that the flora in the environment in terms of body flora does simplify.

How does it simplify? Does it simplify toward the gram-negative side of the house or toward the gram-positive side of the house? --which means, gram-negative being your potential disease carrying organisms and your gram-positive being the friendly guys which we live with every day.

If we look at this chart, then, we can see that-- Again, before I get into that I want to continue with just one curve here which, in terms of the total number of organisms, even though they decreased, the organisms that did live, the eight or nine categories, as you can see here, increased. There was an increase in growth with respect to the ones that were there.

Which ones were they? If you look at the green line here, they were the gram-negatives, and the gram-positives were on a decline. Now what this really means in terms of us in the space business -- and this isn't any new phenomena: we have known about the potential of this phenomena through other studies, such as Boeing and some of these things: this merely verifies it. In fact, in an active life support system you may think at first thought, since this was a passive life support system, that maybe this won't occur in an active life support system. But I think if you look at the data this won't be borne out. It will show that this crossover actually occurs faster with respect to an active life support system than it did in this particular passive life support system.

So, again, what this means in terms of us in the space business, when we're talking about long duration missions maybe we ought to know more about this phenomena if we're talking about rotating crews, and taking crews up and putting them in this kind of an environment that maybe have a high concentration of gram-negatives in the environment.

If we go one step farther and say "What were some of those potential pathogens in the environment?" -- we can look at-- In our pre-mission testing we picked up what we call beta-hemistrat, which is a common throat organism. And if we look at the red chart here that's a staphoreous which you see, or have heard about in hospitals and is very common, and is known to have caused considerable numbers of deaths with respect to that. And in the operations if this kind of aninfection occurs it could cause -- lead to death. And this organism showed up on this particular individual, not in the pre-mission testing. It doesn't mean that it wasn't there; all it means is that with the sampling techniques which we used prior to the mission we did not detect it. However, once in the mission, we detected it continuously on this particular individual throughout the mission.

Again, with respect to the beta-hemistrat, we detected it throughout the mission on this particular individual, but there was some transients that occurred. There was some transients in this particular organism that occurred. We only identified these particular organisms down to the general level. We did not take them down onto a deeper level, which may in essence have led you -- identified how these transients occurred. We didn't have the funding to do that kind of a thing. We were lucky to get down to this particular level in this effort.

If we look at, then, some of the results that we got out of this: we did identify

(End of tape, Side 1)

(Side 2)

-- that we used in the environment, so we were able to control it to a certain extent.

Okay. If we go in to look at the recommendations in terms of we ought to maybe know a little more about how this transference occurs, we ought to know certainly something about what this bio-shock phenomenon does to us in terms of the space station work, we ought to know what the criteria is in terms of crew selection or how you -- well, what I'm talking about here is in terms of when you know an individual has these kinds of strep -- or staphoreous do you really put him on a mission; like in this particular case the individual had it before we even went.

And we should establish some cleansing criteria for the environment.

In terms of the maintenance area, if we look at the work that we did there, we started out with the objective then of determining total maintenance workload and then measuring the effects, if we could, of long durations in terms of actually performing the maintenance, and then evaluate two maintenance prediction techniques with respect to crew time to perform tasks. And these were Method 2 and Method 3 from the 472 document, which those of you who are familiar with maintenance will be familiar with.

In terms of total man-hours used, there was 1860 man-hours available throughout the mission for work. 321 hours of this particular -- of these 1860 -- was used for maintenance.

How was this maintenance distributed, then?

Around 20 percent of the -- if we look at it at the beginning of the mission was throughout the mission but had narrowed down to about 14 percent. We did have quite a few malfunctions that we left undone at this stage because we knew we were -- the mission was over and those functions weren't real critical to the success of the mission.

I think we did a lot of cannibalizing in this particular mission. We did not have a lot of the parts, and we were very fortunate to have some of theindividuals on board, like this Man No. 4 who had a wide experience in electronics maintenance where most of our failures occurred. It was not planned this way, it just happened this way. I think we can't afford to let that happen with space stations; we want to make sure that we have those skills on board.

You can see here that most of the maintenance load was taken up both in the scheduled and unscheduled area. I might say that we had 13 unscheduled tasks and 13 unscheduled tasks which we had made a failure mode analysis effects -- a study for prior to the mission that we went on, and we had predicted spares and we had predicted what failures would occur in these areas, and we also had certain scheduled maintenance that had to be done on the equipment. But the highest majority of the failures that we did encounter were not predicted failures.

VOICE: Did the scheduled failures occur?

MR. MAY: Well, yes, the scheduled did. It was in the unscheduled area. We did the scheduled ones automatically. We knew before we wenton the mission we were going to do the scheduled ones. They were preventive type maintenance or inspections, and this sort of thing.

I didn't show the prediction chart because it is a sort of complex chart and it takes time to go over it. But in essence it showed that Method 2 came out to be much more valid in terms of prediction, predicting time to do maintenance than Method 3. Method 3 was pretty much all over the board.

But in terms of the results of the mission the maintenance work load was equivalent to one man. The task times were not-- What I'm saying here is, as far as we could detect we had no maintenance failures which affected the safety of the crew and, therefore, the tasks that were done were routine tasks or unscheduled tasks which could be put off at a certain time to when the crew members had the time to do it and, therefore, there was no delta that we could discern with respect to performing maintenance in this environment as there was in the dockside case.

We did have correlation with the method 2, as I said. And, again, we think that the unscheduled -- without the maintenance that we did do, certainly we would not have completed the mission. We did have a failure which occurred on the third day that was critical to the mission success, and if we had not done that particular task I don't think we would have gotten any farther. Our commode went out.

VOICE: Is this one man--

MR. MAY: It's equivalent to one man.

VOICE: -- and eight-hour day, or three shifts, or what?

MR. MAY: No. We had 1860 hours available for work in the mission from the six men. There was 321 hours of that available work time that was used up in maintenance alone. It's about a ten-hour day, I think, for each individual, a ten-hour working day.

Again, here, in terms of recommendations, certainly we should consider maintenance skills when we are selecting the crew. And from the standpoint of -- I think in these kind of studies there are a lot of other things that we could learn with respect to maintenance.

If we look at, then, the mission planning and things, without going into some of these problems, we're all familiar -- Jacques mentioned several of these problems I think in the command decision area. We had our first problem--

As you know, when we went on the mission we had a three-day -- we were going to make this a three-day test dive, and at the end of three days if everything went right, the specific equipment was working, we were going to make a decision to go ahead with the mission. That mission go-ahead decision was made three days into the mission.

We did have failures in the scientific equipment in terms of the sub-bottom profile and in some things like that, but we decided to go ahead with the mission anyway rather than go back.

There was another major decision in terms of surfacing -- resurfacing it for tow, a major decision with respect to shot loading, CO build-up, and mission extension. These all caused, I think, some interrelated problems with respect to the internal aspects of the mission as well as the relationship with the topside.

If we look at some of the complaints, then, that we got relative to these, you can see that complaints with respect to communication with the surface crew continually arose throughout the mission. And these points here were such as a tow day, the CO build-up. This is dive day. And this is the command decisions. So you can see that complaints with respect to the communication with the surface did increase with time.

We didn't-- Again, we noted these areas and we noted these problems, and from this I think the recommendations can show that we need explicit definition of what the mission control -- from what the surface ship -- what decisions they have the prerogative to make, and what decisions that you make within the environment to establish the clear limits in terms of what consists of abort criteria, and in order to continue the mission, and who makes the decisions; to establish, again, realistic crew workload distribution.

I would say now we're talking about a seven-day workload with respect to crew members in space stations, and we say -- we use as this justification, if you're up there with a bio-science payload you're up there with astronomy, you can't just leave those animals and go off. You've got to work seven days. But I'm not saying in this kind of a thing that we let the animals die, we all take off on one day. But I think the workload distribution, when we start programming a seven-day work-week we ought to think long and hard. Because I don't think the philosophy holds up that up there you feel that the guy is going to be less bothered if he is continuously busy all the time. I think that he's not going to have any problem in taking a day to relax if the recreation in the environment is such that it gives him an opportunity to do other things.

In our particular case we had no recreation to speak of which gave us an opportunity to do other things. But certainly I wouldn't have squawked about a day off to have read and relaxed, and have taken a shower and just reminisce about the things that I had done through the mission, plan the work that I was going to do from there on. Short term goals I think are very important from the standpoint of really -- of your workload, trying to set up short term goals so you can see meaningfully how you really accomplished your job and how you are progressing in getting the data that you're talking about.

I think in our case we had a communication problem with the family. And we had communication problems, period. I'm not sure that this exists in the space station, but it is certainly something that we ought to consider. And in the space station we can certainly give other means of communication, such as TV and things like this, that we couldn't have here. But I think it seriously ought to be considered with respect to space bases.

I want to talk now just to some -- and I know this is sort of a busy chart, but I want to talk from the standpoint of the way I felt about certain things and these particular factors, in terms of the recreation, work, sleep, gym -- the gymnasium kind of activity, the physical exercise, the personal hygiene, the hot shower, sauna bath -- anything in these areas, I think. They can cause problems with respect to your performance as time goes on. They get more critical I think as time goes on, and as you are in this environment this time.

I think certainly with respect to the recreation and the kind of a lay-out we need in space stations, I don't think going in to some sort of solarium, such as having greenery around, and these kind of things, is beyond going into the space station design and trying to come up with places where the guys can sort of isolate themselves and think about their problems, and some of their work, and things, in a very relaxed kind of an environment.

And the crew make-up.-- Certainly I think that the scientific and the engineering kind of crew make-up is going to be different from the kind that we have been used to selecting in the space program. We're talking about here men who -- certainly motivation I think ought to be one of the key criteria with respect to selecting. And compatibility can't be neglected. But motivation I think certainly should be the primary one.

In the command and the hierarchy: this has to be laid out and documented, as I said, very thoroughly. We had problems on our mission, and I don't think we want to run into these same kind of problems in the space program.

I think out of the whole mission, out of the design things that we can change in the space station area, to me I think the toughest factors that it's going to be to compensate for is going to be the social isolation and confinement in terms of isolated from your social environment, which is tough to do anything about; isolation from the environmental stimulis, such as the earth, the trees, things that we take for granted every day; and in terms of confinement, not being able to get up and get out of the particular environment that you're in, not being able to go out and talk a sun bath or a swim or something like this. I think it's going to be tough factors to design for in the space station area.

Again, I say that in the studies that I think -- I can't over-emphasize the combination, the synergistic effect of the combination of the simultaneous removal of the individual at the same time from both his social environment and from his environmental stimuli. I think if you remove me from my family and everything and you put me back on top of a hill somewhere I think I can live there a long time as long as I had the sun and the trees and this sort of thing; not that I wouldn't miss the family life, but I don't think it would bother me near as much as if you take both of them away and then put me in an environment to do -- where I've got to do creative work, where I've got to do scientific work, and not just do a repetitive kind of an operation which doesn't require you to think, and this sort of thing, but requires you to be a stimuli and to be a creator within particular problems, and handle your data in certain manners within that environment.

I think, again, as I've pointed out throughout this presentation, that environmental monitoring, knowing the status of the environment, is certainly I think a source of stress that we ought to consider in terms of the psychological stress as well as the physiological well being of the individuals.

The maintenance on scientific equipment we haven't really considered, I don't think. We are starting now to consider the maintenance problems that we're having with the primary system and subsystem in the space station. But I think we're sort of overlooking the scientific -- the equipment and the maintenance that you have to do on the scientific equipment. This is certainly a source of stress from the standpoint if a guy, if a scientist has put two or three years of his life, or five years or his life into getting that equipment up there, and he's there to operate it, and the stuff malfunctions at the very beginning of the mission, and he has got to stay there for six months and no equipment to work and no way to repair it. it can be a very frustrating problem, but one I think we've got to reckon with.

In general I think I can conclude that in the underwater systems we can utilize these kind of systems as space station analogs, specifically, in the ones that I've seen, in the man related areas, if we maintain a real mission. If you go with a simulator I think, again, the data isnot really interpretable in terms of an analog. It may be a simulation but you must recognize it as a simulator and not as an analog.

You must maintain a real mission in the systems, as I said. When I say "man related" what I'm talking about is the hardware that we used and the hardware that I've seen is not space hardware. There are certain areas, such as in the life support area, where there are common requirements, common functional requirements such as maintaining and sustaining life, which I feel if recognized that we could meet both requirements in both the ocean system and the space system with the same kind of equipment that we're going to use in the space station and develop operational capability and confidence in that equipment which could lead to actual space-type hardware. And I think in meeting those same requirements for the ocean, it could be a very cost-effective way of doing the job.

Again, I reiterate what I've said throughout this mission, thatwe were dealing with oceanographers, and oceanographers were not particularly conscious of revealing things and information about problems and data that we wanted for the space problem. I think in any of these areas that we get involved in, we ought to try and have a NASA man on board.

That, gentlemen, sort of concludes my part of the presentation. Again I want to iterate that in looking at the BEN FRANKLIN what we've said here, we've try to say in light of the way we would like to see things changed, or done in the space station area. We have not said it as being reflective on the BEN FRANKLIN vehicle itself, because it is a very nice submersible vehicle.

I thought I had better end up with that note.

VOICE: Any questions?

MR. MAY: We're open for questions, Jacques and I both.

Go ahead.

VOICE: I take it that most of the time you were in darkness?

MR. MAY: No, that isn't true. We had our lights on a considerable amount of time.

VOICE: But I mean the ocean outside.

MR. MAY: Well, it's 600 feet, and you can see fairly well. We followed tuna all up along north of Cape Hatteras. Jacques and I lay on our bunks and watched these tuna go around and around the boat north of Cape Hatteras. And then when we went down to 2000 feet we always had lights of some sort. We hadthree different sets of lights.

VOICE: What I was leading up to was the type of cycle you followed, or the routine, rather, that you followed. Did you follow a regular day-night schedule or--

MR. MAY: No, I don't think we did. I think a few of the men did. I think particularly some of the men felt that they wanted to stay on the same cycle that they -- the diurnal cycle that they were on prior to going down.

I myself, and several other guys, changed very quickly, without any problem, to a night work schedule, and with no problem at all. I did have problems when I came back up, getting back used to the day-night cycle on the earth side. In fact, the first night I was up I didn't sleep a wink.

VOICE: Di most people seem to stay on the same cycle once they got used to it?

MR. MAY: Yes, they did.

VOICE: What kind of a cycle was it, Chet? What happened to the days? What did you schedule, and how did it end up?

MR. MAY. Well each one of us, I think -- we had our own schedule, our own work. And as long as we did that work within the mission profile -- I mean within the time period, then we were in good shape.

VOICE. What was the time period?

MR. MAY: Okay. Just let me finish here.

On the dive days, of course, at the bottom, we had specific things to do, and everything else took second priority. That took first priority. So when we were on the bottom everybody was concerned with looking out for unseen objects, and we were doing the oceanographic mission there. But when we were in a normal drift kind of cycle I had my routine and the Navy guys scheduled blasting caps to get their acoustic work done, and Jacques scheduled his plankton sampling, and things like this, whenever it fitted his schedule.

There was an eight to ten-hour period of which only three guys at a time were up. There was about a six to eight hour period where all six of us were up.

VOICE: Okay. There was no fixed period of time, then. It was pretty much ad lib depending on the time; it wasn't so much eight-on and four-off sort of thing?

MR. MAY: No. It came out to be like this: three men went to bed normally around six to eightin the morning and slept until one to five in the afternoon, depending on what time they went to bed. And three men went to bed around ten -- somewhere between ten and twelve, and got up somewhere between six and eight. So it was this time between six and ten at night that we were all up doing whatever--

VOICE: But you were on a twenty-four hour cycle?

MR. MAY: We were on a 24-hour cycle. Someone was awake at all times.

In my own case, I spent longer in the sack toward the end of the mission to get the same amount of rest that I did at the beginning of the mission, as much as three to four hours longer.

VOICE: You weren't on a 24-hour cycle. You told just told us that in your last sentence. You shifted your cycle. You lengthened it or something.

MR. MAY: There were men up the full twenty-four hours.

VOICE: Your work cycle was twenty-four, what you set up for yourself, but your diurnal -- your circadian rhythms had shifted, because you just told us that.

MR. MAY: We didn't stay awake twenty-four hours, no. Each guy got his eight to ten hours sleep.

VOICE: You don't understand, I think. I said your natural rhythms.

MR. MAY: Oh, yes.

VOICE: The work cycle was twenty-four.

MR. MAY: Right. Right.

VOICE: How about eating? Did each individual just eat when he felt like it, or did the whole group--

MR. MAY: Toward the end of the mission it got to be that case. At no time during the mission did all six guys, except the first day, and we dispensed with that idea because of volume and that sort of thing. --of eating meals together.

But toward the end of the mission the meals -- the individuals started tending to eat more meals alone than they did at the beginning of the mission.

VOICE: Did everybody fix his own?

MR. MAY: Everybody fixed his own. Everybody cleaned up after himself. Unless you made some arrangement with your partner: he fixed one day and you fixed the next day.

VOICE: It's interesting. What you're saying is mealtime did not become a time for discussion or planning or anything of that nature.

MR. MAY: News time became--

VOICE: Noon time?

MR. MAY: News time. We got our news every night at eight-thirty. And the only time during the mission which the whole crew set down together to talk, to B.S. or anything like this, was around between eight-fifteen and nine-fifteen at night.

VOICE: I'm still confused about this cycle. I thought you said you were sleeping longer toward the end of the mission. But still you said that you did stay on a twenty-four hour cycle. And yet I understood you to say it was not a twenty-four hour cycle.

VOICE: The work cycle was twenty-four hours. In other words, they planned their work around it. But their own diurnal rhythms shifted. He just told us that.

VOICE: (Unintelligible) --repetitive.

MR. MAY: It was more or less repetitive on a twenty-four hour--

VOICE: The percentage of the day you were sleeping changed, but it was still a twenty-four hour time.

MR. MAY: Yes, it was still a twenty-four hour time.

VOICE: Let me try and explain it another way.

Before the mission we broke the-- we have six men. We made three two-man teams. Jacques and Kaz was the No. 1 team, Ebersole and Chet was No. 2 team, and the two Nav-Oceano scientists was No. 3 team. The oceanographic people could adjust their cycle by themselves, because they had to spend their time on the oceanographic experiments. So they were planned for twelve hours on and twelve hours off, or they could make it eight on and four off, as long as one or the other was available to take care of their oceanographic equipment.

Jacques was working with his oceanographic experiments as well as he worked with the piloting of the vehicle. Don Kazmire worked with Ebersole who were also -- they were two pilots. So they had to work their time in with Jacques. And Chet had his own schedule which he could work in on maintenance activities and other activities, but it was flexible.

What we're trying to get across, we had a twenty-four-hour work cycle, but there was a great deal of flexibility worked into the over-all program.

VOICE: But you weren't working at twenty-four hours and living at twenty-six or twenty-eight hours?

MR. MAY: No, we stayed pretty much on a twenty-four basis.

Certain things that I had: most of my tasks were repetitive in three-day cycles. In other words, on Day-1 I did certain things. Maybe I concentrated on microbiology. Maybe Day-2 I could concentrate on contaminant analysis in the environment. Maybe Day-3 something else. And on Day-4 I went right back to the microbiology. So that I had my own cycles, which if I did all the activity that I planned for that day and that was the end of the day, I read the rest of the day or did some sort of recreation to finish out till six o'clock when I went to bed.

VOICE: If you had your schedule completely at your disposal you might stretch out your twenty-four hour into a 30-hour schedule, mightn't you?

MR. MAY: I see no reason why you couldn't. Because we didn't pay any attention to really night and day kind of business.

VOICE: Obviously there are schedule limitations (inaudible)

MR. MAY: Yes. We tried to go pretty much on a twenty-four hour cycle, though, I think.

Bob, did you have a question?

VOICE: (Inaudible)

MR. MAY: Toward the front of the mission I was sleeping around seven to eight hours, toward the first two weeks. Toward the end of the mission-- And that's about what time I was spending in the bed. Toward the last couple of weeks of the mission I personally was spending somewhere around eleven to fourteen hours to get the same seven to eight hours sleep that I was getting toward the front of the mission.

VOICE: (Inaudible)

MR. MAY: Yes.

VOICE: (Inaudible)

MR. MAY: Oh, yes. I think the information from the outside world was very narrow to us, specifically. And I think in some cases toward the end of the mission it was beginning to bother some of the people.

VOICE: (Inaudible)

MR. MAY: Continue to increase-- You think the time in bunk would have increased? I don't know how it could increase much more.

VOICE: You'd get bed sores.

MR. MAY. Yes, I'd get bed sores if I stayed there much longer.

VOICE: We've been working on the assumption that implicitly, that constancy is good. Did you find you wanted variability and flexibility, or that constancy was boring?

MR. MAY: I think variability wouldhave been the way I would have gone, some way to make things different. I think variability in terms of food, variability in terms of the personal hygiene things that we did, and this sort of thing would have been much better than-- and variability of recreation. For example, I like to play poker every now and again. We had to train two of the guys to play poker. And it took us three weeks to get a poker game going. And when we got it going we all enjoyed it, the ones that played. There was four of us that played. But the two guys we taught took all the money home.

VOICE: Do you have any way to evaluate the performance-- I know it's very difficult. You're dealing with (unintelligible) scientific activity. But is there any way of getting any evaluation of the performance of the various team members other than a psycnomotor tester? In other words, did they do everything you wanted them to do (unintelligible) could have done under more favorable circumstances? Or was there a penalty, or price they paid because of the environment in which they found themselves?

MR. MAY: Well Ithink there's a way, Stan, in which you can get to that data, or get better data in terms of performance and in terms of the tasks that were accomplished, the number of tasks and the data that was set out to be gotten as compared to what was actually gotten, and the reasons as to why. But I think you have to have very good cooperation and understanding from the guys who are actually doing that, to get to it.

They've got to be willing to spend some of their time to document it. And I think in an operational situation if they're not really aware of the space program and the requirements of the space program they're not really too willing to do that.

VOICE: It's very difficult. I was thinking, of course of the Picard case where he used time in the water as a criteria. But it's not a good criteria, because you still don't know how productive he is in water.

MR. MAY: Exactly right.

VOICE: So it's very difficult to come up with an objective. I thought maybe you had something that you might use.

MR. MAY: We've got times -- and, Matt, you might want to say something about this, with respect to the different tasks. We attemped to do what you're saying, I think.

VOICE: Yes, we measured the times in which -- like the oceanographic tasks that perhaps you'd be interested in.

And the oceanographers, we paid particular attention as to their time on station and what we could observe as working. And we have that data to review. I'm sure we can talk about it.

But according to the output, which we haven't shown here in this presentation, they accomplished their mission goals. They did everything they set out to do.

MR. MAY: Except where they had a couple of failures of the equipment.

VOICE: Except where they had failures, which they could do nothing about.

VOICE: I'm thinking of the crew interpersonal relationships and whatever it was that led you to say that another time they ought to have a more specifically structured set of responsibilities, and so on. I'm looking for the difference between three men and six men, and whether you would expect to see a qualitative difference in that area of things between the three-man crew and the six-man crew.

MR. MAY: I personally would say that, of course, when you're talking about a three-man crew and a six-man crew, obviously you're talking about a different ratio of scientists and engineers. And I think if you're talking about all operational guys and all engineers, I think you're talking about a different problem than if you're talking about putting a scientist on board or two scientists on board with an operational guy. So it will depend on-- Really, what you're asking me would depend on the individuals, the background of the individuals involved.

VOICE. What's on my mind, of course, is the experience we'll have in the workshop, and to what extent that is going to throw any light on how we ought to operate when we start putting space stations (inaudible)

MR. MAY: I think our data is more applicable to space stations where we're talking about a higher ratio of scientist to engineer background, crew background. And I think-- If I understand the AAP crew requirements still basically they are operational; if you will, the kind of individuals who are very -- you can put them anywhere and they'll do anything, kind of thing. You still aren't integrating the scientist background where what their work is -- their work, their aims in life are different. They're not really specifically interested in these environments just to go there to see if they can survive or not; they're interested in going there to see if they can get some data. They're notinterested in spending their life's work in getting ready for that job, either.

VOICE: This is one of the things that we think we got out of the program, that there is a difference between the so-called operating group and the scientific type. And the scientist, he's interested in performing certain tasks and coming back with data which will say he has performed a useful scientific measurement.

VOICE: Just as you're talking about this as a way of looking into some space station problems, so also are we thinking of AAP as a way of looking into space station problems. I would like to structure it in a way that throws as much light on how you ought to operate a space station (inaudible) as it can. So I'm interested in probing this area to help us feel that we-- We talked, for example, about the possibility of a second workshop having a period of crew overlap, so we could have six people for some period of time. We're trying to get a handle on whether that's an important thing to try to do or whether it's not.

MR. MAY: I think certainly as you narrow the crew down and you get less number of crews you get a less number of interreactions that can occur between the various crew members, and a less number of kind of group activities that they can be involved in.

VOICE: Let's close the meeting off. Anybody who wants to stay to ask more detailed questions, you're welcome to. I don't want to hold anybody against their will here.

I want to thank you, Jacques, for coming, and Chef, for a very fine presentation.

(Applause)

(End of tape recording)